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Do Enterprise Zones have a role to play in delivering a place-based industrial strategy?

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We examine the body of evidence on Enterprise Zone policy with the objective of assessing its role in broader place-based industrial strategy. We conduct an extensive review, consolidate the empirical evidence into one place and examine it according to economic impact by zone type. We identify two gaps in the empirical knowledge that are systematically addressed through new research on several US Enterprise Zones to gain new evidence on their economic additionality. The article concludes by considering other, hitherto poorly explored, pathways by which zones may help an area to adapt to change and embrace longer-term economic futures.

Keywords: industrial policy, Enterprise Zones, additionality, restructuring
JEL Classifications: O1, O2, R0

Introduction

Enterprise Zones began as an experiment. They are now well into their fourth decade and remain a popular policy option for economic development across the globe. Most recently, numerous governments and researchers have issued statements with newfound or renewed support for Enterprise Zones (EZs), their numerous derivative programmes and place-based policy more generally (Austin et al., 2018; Farole et al., 2018; HM Government, 2017).

The 'return' of EZs to the limelight of economic development policy merits a review of the voluminous body of literature on their impacts. Further underlying this has been new evidence strongly linking economic transformation and economic divergence in the UK, USA and elsewhere to differences in business performance (Economic Innovation Group, 2017; Martin et al., 2018). Additionally, there is a renewed appreciation for the theoretical foundation of Enterprise Zones, which calls for utilisation of economic potential, in broader industrial and regional policies throughout the world (Farole et al., 2018). Both of these research trends now lend further credence to the ability and long-term impacts of thoughtful and strategic industrial policy. The question is what role EZ policy can play in assisting a *comprehensive* place-based strategy.

There is mixed evidence on the effectiveness of zones—it varies according to programmer

governance, the outcome being measured and the evaluation approach utilised (Granger, 2015; Hooton, 2016; Tyler, 2013, 2015). What we need now is new research that consolidates the ample evidence on Enterprise Zones to illustrate the variation according to programmer characteristics and different outcomes of interest, particularly those other than (un)employment levels (What Works Centre for Local Economic Growth, 2016). Moving beyond the question of 'if they can stimulate economic development', research should now seek to understand in which scenarios are they most effective, what their potential trade-offs may be and how can they best fit into a broader strategy that optimises their likelihood of success.

The article does precisely this, offering a comprehensive review of Enterprise Zones, examining how zone effectiveness varies according to programme design and outcomes. Additionally, it moves beyond simply a review by offering new empirical estimates on the additionality of zones according to a variety of outcomes—population, poverty, employment (both resident and non-resident), income and firm/establishment creation—using well-established evaluation techniques and comprehensive controls in three case studies. The new empirical work confirms the conclusions of the review through systematic variation of environments and opens discussion on zone governance variations. From these first two contributions, we then seek to push forward the conversation on zones, their new form/model which emphasises investment attraction and their role as part of a broader development strategy (Granger, 2015).

We bring together the extensive evidence base that now exists on the impact of Enterprise Zones around the world and find that, on balance, they can help build economic momentum in an area, but that there is little evidence that they can change the absolute growth/development trajectory of an area on their own. They can add positively to the margins in areas undergoing absolute growth/development and slow the decline in areas undergoing absolute decline. Furthermore, our analysis shows that areas targeted by place-based policy initiatives (i.e. areas with zones) can indeed 'swim against the current' by exhibiting positive economic growth even in cities with overall economic decline.

However, one of our main observations is that traditionally the assessment of zone impact focuses on *short-term employment* demand creation effects on local areas and thus often emphasises short-run displacement effects. In relation to the needs of a local area industrial strategy, it is also important to consider the *longer-term contribution* that zones can make to local economic development when they are delivered as part of a coherent land-use *restructuring* plan for the areas concerned, which enables them to transform their physical infrastructure, particularly in relation to connectivity. Previous evidence, and the findings presented here, point to the long-term role and impacts of zones, through sustained, year-on-year, improvements to an area for a variety of outcomes. The evidence speaks to zones' role as this sort of long-term restructuring tool, and we conclude the article by considering hitherto poorly explored pathways by which zones may help an area to adapt to change and embrace longer-term economic futures.

The second section presents an analysis of the existing evidence. The third section details the article's empirical methodology, and the fourth section presents overall results. The fifth section moves into the discussion on EZ's role in broader local development strategies. The sixth section concludes.

Literature review

The EZ concept originated from Peter Hall in the UK in the late 1970s and reached widespread prominence, principally in the USA at the state level in the 1980s and later at the

national level in the 1990s (Ferrara, 1982; Hall, 1981; Hirasuna and Michael, 2005). They generally seek to stimulate economic activity by removing barriers to markets and businesses through government/policy action, such as through the elimination or reduction of taxes and zoning restrictions (Ferrara, 1982; Hirasuna and Michael, 2005). Foundationally, Hall and others conceptualised them as areas where regulatory structures were relaxed to open up activity. Practically, individual governance structures of the various zone-inspired programmes across the globe may utilise both regulatory relaxation and a reorientation of local regulations towards particular goals. The theory recognises that government barriers may not be the only or even primary causes of economic laggardness, but still argues that the removal of governmental barriers may be enough of a catalyst to overcome any other barrier as well (Ferrara, 1982; Hirasuna and Michael, 2005).

The largest EZ implementation is the US federal programme, which has created 2145 unique zone locations (of any type/version) in the country, since its start in 1994 through 2011 (HUD, 2018). Approximately 18% of those were/are located in urban areas, 80% were/are in rural locations and the remaining 2% are not classified either way (HUD, 2018).[1] In addition, a Housing and Urban Development (HUD) report in 1991 documented an additional 2260 state enterprise zones in 34 states (HUD, 1991), and more recently, Beck (2001) identified 3500 unique zones (federal and state) in the USA. In the UK, there are several dozen EZs, and internationally, there are countless more zone programmes, such as the *Zones Franches Urbaines* in France (Ministry of Housing, Communities, and Local Government, 2018).

The full literature set on EZs and their impacts is rich and nearly four decades old. On the theoretical side, Butler's *Enterprise Zones: Greenlining the inner cities* from 1982 laid out the theoretical foundation and rationale for enterprise zones, building out the argumentation underlying why zones can and will help revitalise depressed micro-geographic areas. The earliest relevant empirical evaluation is a study of Virginia's state EZ programme in 1984 from the Virginia Department of Housing and Community Development (1985).[2] Building on these works, there have been dozens of studies on EZs themselves, and many more on place-based policy and other related topics with direct relevance for EZs.

While the full set of literature on, or relevant for, EZs is quite extensive, the authors have identified only 61 pieces of research that have conducted empirical evaluations of EZs or that have conducted a review of empirical research with an overall conclusion on zone effectiveness—a substantial number, but a relative paucity of evidence given the large body of potential case studies to draw upon.[3] Of these empirical studies, 28 (46%) reached a positive overall conclusion on the effectiveness of zones, 16 (26%) concluded zones were unsuccessful/ineffective and 17 (28%) concluded zones had mixed impacts. However, this breakdown is an oversimplification because many of the individual reports examined multiple case studies and multiple outcomes. As illustrated by Tyler (2013), analysis of zone impacts should not be limited to binary conclusions, but rather help understand how and why zone impacts have varied.

In fact, the authors have determined that within those 61 studies there were 144 unique evaluations of 36 case study locations across multiple countries and zone types. This includes assessments of the national programmes in the USA, the UK and France and dozens of subnational locations. Table 1 provides a list of these case studies, and the online Supplementary Appendix provides a full list of research titles.

Within these evaluations, evidence on EZ effectiveness has been largely mixed and contentious, with methodological and theoretical critiques lobbed at both sides. Indeed, the studies at face value can have directly

contradictory overarching conclusions on zone effectiveness. This is most clearly illustrated by the pair of studies from Busso and Kline (2008) and Busso et al. (2013), which found positive results, and the pair of studies from Hanson and Rohlin (2011, 2013), which found unsuccessful and potentially negative results, despite them all examining the same programme and using similarly sophisticated methodologies.

We illustrate this variation more clearly in Tables 2 and 3 by breaking down the body of empirical evaluation literature on EZs by their finding for specific outcome measures. This simple exercise reinforces the observation on the mixed evidence on EZ effectiveness. Across all metrics, there are varying degrees of programme success according to the empirical evidence that exists. As the What Works Centre for Local Growth critically pointed out, employment is the most prominent outcome measure in the EZ literature, with 38 instances of analysis in the 144 evaluations reviewed. Of those studies that examined employment impacts, 58% found EZ programmes to be effective, while 11% found them to have negative impacts and another 26% found them to simply be unsuccessful. Analysis of firm impacts is also prominent, with evaluations on new firm creation, existing firm growth/expansion and firm locational choices comprising 35 instances of analysis.

Despite the variation in measured performance, across the different metrics that researchers have analysed, only two demonstrate less than a 50% success rate among empirical evaluations—EZ impacts on industrial activity and EZ impacts on existing firm (re)location decisions. Again, this is from a comprehensive body of empirical work, but from a relatively limited number of case studies. The authors interpret this as evidence that EZs are largely successful even across different case studies, methodologies and outcome metrics, but that success has been far from assured in programme implementations up to now.

What are the reasons for this variation? It may be attributable to some degree to

Table 1. *Case study locations from empirical EZ research*

Case study	Number of studies examining impacts in case study
California	18
New York City	11
USA	10
Atlanta	9
Florida	8
New Jersey and UK	7 in each
Baltimore, Pennsylvania and Virginia	6 in each
Chicago, Detroit and France	5 in each
Indiana and Philadelphia	4 in each
Cleveland, Colorado, Kentucky and Maryland	3 in each
Connecticut, Danville, District of Columbia and Ohio	2 in each
England, Europe, Los Angeles, Louisville, Lynchburg, Manchester, Newport News, Norfolk, Paris, Portsmouth, Roanoke, Saltville, Swansea and Texas	1 in each

Table 2. *Number of empirical studies by outcome measure and conclusion*

	Industrial activity	Commercial activity	Employment	New firms	Firm expansion/growth	Firm (re) location	Income	Property	Capital investment
Mixed	2		2	1	2		1	1	1
Negative	1		4	1	3	5			
Positive	1	4	22	9	6	5	7	8	6
Unsuccessful	1		10	1	1	1	3	1	1
Total	5	4	38	12	12	11	11	10	8

Table 3. *Percent of empirical studies by outcome measure and conclusion*

	Industrial activity	Commercial activity	Employment	New firms	Firm expansion/ growth	Firm (re) location	Income	Property	Capital investment
Mixed	40%		5%	8%	17%		9%	10%	13%
Negative	20%		11%	8%	25%	45%			
Positive	20%	100%	58%	75%	50%	45%	64%	80%	75%
Unsuccessful	20%		26%	8%	8%	9%	27%	10%	13%
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%

Note: Column totals may not exactly equal 100% due to rounding.

methodological approaches, including data restrictions, inappropriate or insufficient counterfactual approaches and other issues.[4] However, the authors propose that researchers have already largely answered this question, if not directly so.

First, Kline and Moretti (2014) provide a theoretical explanation for variation in EZ effectiveness. They argue that, even with the existence of geographically concentrated and inequitable market imperfections, it does not automatically imply that corresponding spatial targeting of policy is the best option; there can be tradeoffs between the welfare of the targeted area(s) and elsewhere. Variation in programme performance is, perhaps unsurprisingly, inherent and based largely on local contexts, which will strongly influence specific outcomes.

Second, policy evaluators and researchers have clearly noted a set of lessons from individual programmer evaluations related to why EZ performance varies. If Kline and Moretti's observation explains why performance variation exists, these more specific lessons explain how positive EZ impacts can be maximised. We summarise them as follows[5]:

Targeting

- Zone expansion can produce mixed results or lessen results of existing zones (Greenbaum and Bondonio, 2004).
- Zone performance varies based on industry mix within zone (Kolko and Neumark, 2010; UK Department of the Environment (DoE), 1987, 1995).
- Incongruous zones are not effective; zones should be a contiguous area (Briant et al., 2013).

Outcomes

- Zones help creation of new businesses, but more likely to have negative or unsuccessful results for existing businesses (Givord et al., 2013).
  - Zones can cause displacement effects (Hanson and Rohlin, 2013).
  - Zone effects typically become highly capitalised into rents, thus benefitting owners of property more than renters (Bond et al., 2012).

Sustainability

- Zones are unlikely to produce long-term, significant, sustainable impacts on their own (Neumark and Simpson, 2014).
  - Effects often lessen over time (Givord et al., 2013).
- Zones can cause capital substitution for employment over time (Lynch and Zax, 2011).

Governance

- Institutional capacity matters and greatly affects success (Tyler, 2013).
  - Evidence points to greater success in large cities where programme management has more resource (Rogers and Tao, 2004).

We argue that, despite the sizeable body of research on EZs and their long-term popularity, many have tended to think in binary terms, ignoring these more nuanced lessons around EZ effectiveness. It is not a question of "Do Enterprise Zones work?". Zones can and do work when applied in optimal situations—ones which consider their foundational theory, strengths and weaknesses. However, zones also potentially have very real trade-offs that must be weighed by local policymakers according to local contexts (e.g. would employment gains and capital inflows justify potential displacement and programme costs?). Furthermore, there remain critical questions on how environmental factors and local governance resources impact performance.

These considerations must be made methodically, and policymakers must clearly understand how zones can supplement a broader local development strategy, one that draws on the EZ's strengths to maximise the potential for the local strategy's success. These considerations also point out the one glaring gap remaining in the empirical literature: what are reasonable expectations for policymakers in terms of outcomes over time?

Thus, the goal of our empirical work is to delve into the additionality of zones. Having examined zone impacts reported in the literature by outcome and area types, we find little analysis on how much impact policymakers can reasonably expect to achieve through EZs. A comprehensive impact assessment of every zone is beyond the research remit here, so we argue the right approach is a study that examines multiple outcomes over time in multiple zones in varying environmental contexts (i.e. citywide conditions), all while using well-established evaluation approaches. This serves three purposes.

First, the article's proposed empirical evaluation approach provides affirmation that zone performance varies according to a variety of factors. There is clearly variation in zone performance reported in the literature, but the variation could simply stem from the numerous methodologies and case studies employed across individual projects. The authors have found no research that has utilised systematic variation in a comprehensive manner in its evaluation approach to conduct an empirical assessment of Enterprise Zone performance. By drawing on the accepted lessons and techniques already developed in previous research, the authors can tick the proverbial box on the existence of performance variation. In other words, the exercise confirms the inability to reach binary, conclusive results on Enterprise Zone effectiveness, at least when limited to data-driven evaluations that fail to account for governance variation.

Second, the empirical analysis allows us to draw new lessons, specifically on how zone performance varies in different environmental conditions. The three case studies offer clear and different local development paths that control for broader trends. The various metrics conducted in multiple counterfactual scenarios control for methodological variation. The geographic treatment strategies control for displacement effects and target-specific characteristics. Together, the methodological variation enables new evidence on the additionality of zones as well as some of the potential trade-offs that may arise for a locality. This new evidence more directly answers the question of what are reasonable expectations for zone impacts, both good and bad.

Third, the empirical exercise allows us to examine more closely the governance variation

of zones and how they can be used to secure longer-term economic development objectives later in this article. By confirming zone performance variation, systematically testing environmental factor variation, and developing better measurements on the additionality of zones, the article moves the discussion to how to maximise the likelihood of zone success in the penultimate section.

Empirical strategy

Case studies

The US Federal EZ programme is a logical choice for the research in this article, given the programme's popularity as a research topic, its documented targeting approach and its extensive implementation. With its four sub-types (Empowerment Zones, Renewal Communities, Enterprise Communities and Enterprise Zones), the programme offers options for temporal and spatial variation while maintaining policy similarity.[6] In addition, the zone concept has inspired numerous local programmes that the research can draw upon to add additional policy, temporal and spatial variation.

The research in this article adopts the popular difference-in-difference (DD) impact estimation model and the empirical approach of Busso et al. (2013) as the basis for its analysis. It adapts and expands these with alternative counterfactuals designed to specifically examine additionality.

We selected our case study cities from the top 50 largest cities in the USA based on population, first identifying those that had both a federal zone programme and at least one other type of area-based initiative (ABI) within its jurisdiction. Among these, the article considered cities with midrange populations, area sizes, densities, gross domestic product (GDP) and unemployment, to ensure similarity in potential resources.

We compared each potential city from 1990 through 2010, which covers the full set of implementation years for the US Federal Enterprise Zone programme, to find cities with differing growth trajectories using population as a proxy for overall socioeconomic trends. Specifically, we looked for cities with each of the following patterns: (i) an accelerating rate of population growth over that 20-year period; (ii) an accelerating rate of population decline; and (iii) a net-neutral population change (from decreasing to increasing or vice versa). Using these criteria, the cities of Detroit, Miami and the District of Columbia met the conditions, respectively.

In total, we analysed four zones. Detroit has two types of zone interventions with overlapping years of implementation and unique geographies. Consequently, the research chose to treat them as two unique zones. The District of Columbia and Miami also had two types of federal zones each, but in their cases, the programmes were transitioned between the types while maintaining the same geographies. Since there were no overlap years for the two in District of Columbia and Miami, the research treated the zones as one initiative per city. We also examined three additional local programmes based on the enterprise zone concept, which serve as a form of robustness test. Table 4 provides the EZ designation type and year of designation for each case study city.

Data

The authors utilised three datasets with full time series coverage and which match the spatial targeting of the programme.[7] The datasets were the Federal Financial Institutions Examinations Council (FFIEC) Census Reports (2013), the Longitudinal Employer-Household Dynamics (LEHD) dataset (US Census Bureaub) and the County Business Patterns (CBP) dataset (US Census Bureaua). It is important to note that the US implemented census tracts nationwide in 1990 and for two of the variables, those from the CBP, we could not find observations lower than the zip code level. We chose eight output

Table 4. *Case study EZ and local intervention years*

	Local intervention	Years	EZ type	Years
Detroit	Renaissance Zones	1997–present	Empowerment Zone	1994–2011
			Renewal Zone	2002–2009
District of Columbia	H Street Retail Priority Corridor	2010–present	Enterprise Community	1994–2002
			Enterprise Zone	2002–2013
Miami	Targeted Urban Areas (TUA)	1997–2012	Enterprise Community	1994–2000
			Empowerment Zone	2000–2011

Note: For Miami and District of Columbia, the zones that were designated Enterprise Communities in 1994 were switched to an Empowerment and Enterprise Zone designation respectively, but maintained the same geography.
Source: Author's elaboration; information from US HUD and the cities of Detroit, District of Columbia and Miami.

variables based on previous measures analysed in the literature and data availability. Table 5 summarises the key details of these.

Methodology

We construct four observation groups for each incentive: (i) a treatment area; (ii) a geographic buffer based on first-order contiguity; (iii) a geographic buffer based on second-order contiguity; and (iv) a non-treatment control area. We geocoded all spatial units within each city to one of these four groups using categorical variables.

The article constructed analysis areas by overlaying maps of each initiative onto maps of census tract and zip code boundaries using ArcMap Geographic Information System software. We marked as the treatment area for each zone, tracts that were either completely encompassed by or partially overlapping the zone. We then marked census tracts that bordered the treatment area with first-degree contiguity as the first buffer control zone and those that bordered with second-order contiguity as the second buffer zone. We assigned each buffer zone tract to the appropriate intervention using indicator variables. In some cases, a unit belonged to more than one intervention and received an additional indicator variable to mark this. We assigned to the non-treatment zone all tracts not assigned to an analysis area or a buffer area. When using zip codes, because they are too large, we use treatment and non-treatment designations only.

Detroit had two tracts and Miami had six tracts with targeting overlap by their zone programme—we marked these units using an additional set of dummy variables. We used the same procedure for tracts with overlap in the buffer control areas. This allowed the research to conduct analysis that both included and excluded the overlap units to ensure any potential influence of double targeting was identified; however, that exclusion/inclusion made no major difference to coefficients or significance. The research chose the more conservative approach and reports results that exclude tracts with double targeting. In those few cases where tracts had multiple designations (i.e. being in a treatment area for one zone and a border tract for another), priority was given to treatment designation and then first buffer ring designation. Every programme has overlap when using zip codes.

The article developed four counterfactual approaches for each policy using its analysis areas: (i) a geographic counterfactual with a DD measure; (ii) a geographic counterfactual with a triple differenced (DDD) measure; (iii) a mainstream counterfactual using non-treatment units with a DD measure; and (iv) a mainstream counterfactual using citywide figures with a DD measure. These are, respectively,

Table 5. *Output variables summary*

Variable name	Description	Dataset	Unit of analysis
Population	Population in the area	FFIEC	Tract
Resident Jobs	Number of jobs in the area worked by residents of the area	LEHD	Tract
Non-Resident Jobs	Number of jobs in the area worked by non-residents of the area	LEHD	Tract
Employment	Total employment (count) of the area	CBP	Zip
Establishments	Number of firms in the area	CBP	Zip
Area Poverty Rate	Percent of area population living in poverty	FFIEC	Tract
Poverty Pct. Total	Area's impoverished population as % of city's total population	FFIEC	Tract
Median Income	Median family income of area	FFIEC	Tract

Source: Author's elaboration.

referred to throughout the analyses as (i) DD Geo, (ii) DDD Geo, (iii) DD Non, and (iv) DD City. Maps 1–6 (online) provide analysis maps for each.

Model

The research calculates its impact estimates using a standard OLS form of the difference-in-difference econometric model. The specifications include common elements from the zone literature and additional controls. The primary term of interest is the coefficient on the binary treatment variable, β_1. The term estimates the mean impact from the intervention among targeted units that has accumulated since implementation. The analysis is for each city and policy separately rather than pooling units together.

The standard model specification for a *naive* estimator uses a binary treatment indicator without area-specific characteristics of the form:

$$Y_{it} = \beta_0 + \beta_1 T_{it} + u_z + v_t + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the observed level of the outcome variable for unit (census tract) i at time (year) t and T is a dummy treatment indicator, which takes the value 1 for treatment in a particular year and 0 otherwise. The coefficient β_1 provides an estimate of the average programme impact on the targeted area accumulated since programme implementation.[8] The term u_z is a full set of group fixed effects for each analysis zone z (i.e. targeted area, first-order buffer etc.) within each city, and v_t is a full set of year fixed effects.

The research used an expanded form of the naive DD estimator that includes controls for current year and pre-treatment tract characteristics. It uses the following form:

$$Y_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 X_{it} + \beta_3 P_{it_0} + u_z + v_t + \varepsilon_{it} \qquad (2)$$

where X_{it} is a vector of observed tract-level proxies for housing, deprivation and economic potential in the year of observation. Additionally, P_{it} is a vector of pre-treatment tract-level characteristics for the start year of the programmer.

The three DD approaches (DD Geo, DD Non and DD City) use the expanded form given in equation 2. The triple-difference approach (DDD Geo) divides each case study city into a target 'zone' (the targeted area and first-order buffer) and a non-treatment 'zone' (the second-order buffer and the non-treatment area) for each policy. The two areas in each 'zone' are differenced and then those measures (the sum for the two zones) are differenced through a triple interaction term. The DDD measure is functionally the same as the DD measure, but with added interactions.

Potential analysis issues

To account for adjustments in tract boundaries over time, the authors used geographic equivalency files to ensure that the tract codes used from year to year incorporate any changes in geocodes and boundaries. The identifying treatment designation used 1990 census bureau vintage geocodes. The master tract lists used for the cities are from the 2010 vintage. The research used a master list file of EZ-designated tracts, the most recent documented list of designated tract geoID's found by the authors, from the US HUD (n.d.).

The research approach incorporates data regionalisation by grouping areas according to targeting, geographic contiguity and area characteristics based on the design of the policies being studied. This allows the research to account for the modifiable area unit problem (MAUP) and autocorrelation issues through the grouping of all units and the use of analysis area means (Viegas et al., 2009).

The research finds the triple differencing approach to be unreliable in its tests and does not discuss the results. We argue that this is related to noise in the data and potential flaws in the approach, which has two documented variations. We do report the triple-difference regression results in our tables.

Results

The authors provide the full regression results in Appendix Tables A1–A3. Appendix Table A4 presents a list of significant results from the regressions by policy, counterfactual and impact. We placed the tables in Appendix to emphasise the broader conclusions rather than the individual metric results.

We find new evidence on zone performance related to four questions: (i) Does success depend on the overall trends of the locality in which it resides? (ii) Can zones affect the economic conditions of an area in absolute terms? (iii) If positive impacts are produced, what are reasonable expectations for them on an annual basis? (iv) Do zones create permanent impacts that remain after the policy has been removed? Broadly speaking, the results confirm that zones can and do have mixed impacts (both positive and negative), which reinforce existing evidence in the literature.

Our analysis finds no evidence that EZs (on their own) will stop or reverse broader decline within a targeted area. Based on the results of our own empirical analysis, the findings in previous literature that EZs have not been able to cause an area that is declining (as measured by the outcome variables) to enter growth. They can slow the decline of an area or accelerate growth in an already growing area, but they do not appear powerful enough to change the fundamental trajectory even if they produce positive impacts.

At the same time, individual outcome indicators in EZs are able to 'swim against the current'. While rare, specific indicators can see absolute improvements even if that same indicator is in absolute decline in the city overall. For example, the poverty rate in Detroit's Empowerment Zone areas had good and significant impacts on poverty rate along with absolute declines (which is good) over the period of observation, even though the city overall had absolute increases in poverty rate over the same period. So, not only can zones cause relative improvements, they can also experience absolute improvements in individual outcomes regardless of what is happening within the overall locality. These results suggest that EZ performance is closely linked to the target area's characteristics and trends, but less so to those of the city.

The range of positive impact estimates from the seven case studies ranges between a few percent of impact up to 227% in the case of Detroit's Renewal Zone.[9] On an annualised basis, the programmes largely range from a few basis points to a few percentage points of impact. These results are in line with what

has been found in the literature for other EZs (see Bondonio and Greenbaum, 2007; Busso and Kline, 2008, for example), and the research argues that such a range is a reasonable expectation for EZs, which are by design small and limited (Butler, 1982; Greenbaum and Bondonio, 2004).

Overall, based on the evidence in previous research and its own, more systematic investigation here, the article finds the impacts of EZs to be generally modest. The scale of their impacts does not negate their efficacy, and indeed, we argue that an accelerated growth or decelerated decline in some outcome of about 1–5% per year across a range of potential outcomes is reasonable, potentially sustainable and a good result for many local policymakers. The key is for policymakers to understand the scale of those potential gains, while working to ensure governance structures that maximise the likelihood of positive outcomes (given local environmental variations produce no clear pattern in impacts).

A path forward—zones in local economic restructuring

One factor that has emerged from our review of the evidence on the achievements of Enterprise Zone policy is that virtually all evaluations have concentrated on the short-term employment creation effects of the policy. In addition, much of the academic critique of zones has considered the displacement effects of zones on employment and product markets or the rent internalisation effects in property markets. It is noticeable that there has been very little discussion of the benefits that zones can bring to the *long-term economic restructuring* of a local economy, particularly given that the evidence base points to the role of zones as an accumulative tool. This is a significant omission if zones are considered as part of a local industrial strategy, where the obvious objective is to integrate a range of activities to secure a reorientation in the longer-term economic prospects of an area. Much of the existing body of zone evaluation evidence has concentrated on the deregulation of zones. However, recent evidence, particularly from the UK, shows that one of their great strengths is to act as a mechanism to bring together the market and the state to shape positive economic change in an area, particularly as it relates to investment in often state-funded local infrastructure. In this context, it is thus somewhat ironic that a concept that began its life as an experiment in rolling back the state in derelict and forsaken areas has proven to have some of its most significant value as a mechanism to facilitate state-enabled investment and accommodative land-use planning at a local level. We conclude the article by indicating how this has worked to good effect in recent British experience.

In 2011, the British Government produced a radical change to the Enterprise Zone model, which offered local government in the UK the ability to finance new investment in infrastructure. The change gave the local development agencies Tax Incremental Financing powers that allowed local authorities to use future uplift in the value of their zone business taxes to finance new infrastructure. As the Government announced (Communities and Local Government, 2011), this 'gave councils a direct stake in the local economy and new ways to support business growth'.

This new feature of British zones was little appreciated at the time, but in reality, it greatly enhanced the ability of local areas to invest in transforming the physical fabric and infrastructure in their area. The new powers allowed localities to accommodate the needs of new and growing sectors—such as those available in knowledge-intensive business services, information and creative sectors—in a way that had hitherto not been available given the limited resources available to them from their own tax base. They were thus enabled to restructure their local economies and embrace a fundamentally different economic future.

As Table 6 shows, local authorities in the UK historically have been heavily constrained in the discretionary resources available to undertake local economic development from central government funding. In the UK, the proportion of tax set at the local level is equivalent of 1.7% of GDP. That compares with 15.9% in Sweden, 15.3% in Canada, 10.9% in Germany and 5.8% in France. In fact, in England, the proportion of total national tax revenues going to local government has actually been *falling* for 40 years, from just over 11% in 1975 to 4.9% in 2012. Simply put, local authorities had a need to make considerable change in their physical fabric and labour markets, but were forced to rely on relatively inadequate discretionary resources from central government.

The 2011 UK Enterprise Zone model changed that situation, allowing a number of local areas in England to use the enhanced longer-term fiscal capacity provided by the zones to invest in new infrastructure to restructure their local economic base. The Birmingham zone in the West Midlands presents a classic case of how the zone policy was used. New land uses in knowledge-intensive business services and the creative industries have become possible in Central Birmingham partly because of new rail High Speed Two rail investment in the area. However, the ability to allow these new land uses had been constrained by the difficulty of removing the 'concrete collar' that had been put around the City Centre to accommodate the use of the car from 1935 until 1963. By using the collateral provided by the land value uplift on Enterprise Zone sites owned by the City Council, it has been possible to address the infrastructure shortfalls and facilitate the longer-term goals of restructuring the economy of the area. It is thus an example of how the new British zone model can play long-term strategic place-based policy.

Table 6. *How British local areas lack tax revenue compared with other countries. Proportion of total tax revenue raised by local state*

Country	1975	2012	Governance model
USA	34.2	35.7	Federal
Canada	42.4	49.5	Federal
Germany	31.3	29.8	Federal
Switzerland	47.3	40.0	Federal
Spain	4.3	42.1	Decentralised
Sweden	29.2	36.9	Unitary
Japan	25.6	24.7	Unitary
Italy	0.9	16.4	Unitary
France	7.6	13.2	Unitary
UK	**11.1**	**4.9**	**Unitary**

Source: Martin et al. (2016).

Conclusion

In this article, we have examined the contribution of Enterprise Zone policy to an industrial place-based local economic strategy. We began by bringing together the relatively large body of evidence on the relative effectiveness of zones as agents of local job creation in areas that have suffered badly from economic decline. We then provided a new systematic assessment of zone additionality to close gaps in that literature and concluded with an illustrative discussion of the zone UK model. We find that zones can reasonably accelerate growth or mitigate decline, but not fundamentally alter an area's economic trajectory on their own. We also find zone performance is closely linked to the target area's characteristics and trends, but less so to those of the city.

Almost all of the focus of the previous evidence on enterprise zones has been on their ability to augment local demand, but we argue that in recent years the emphasis has changed quite significantly. It shows in the UK that the Enterprise Zone policy can be used in a place-based industrial strategy as part of a comprehensive and strategic land-use plan. In the new model, the emphasis has shifted to a more supply-side orientated model incorporating a Tax Incremental Financing approach to local economic development.

Supplementary material

Supplementary material is available at *Cambridge Journal of Regions, Economy and Society* online.

Endnotes

[1] The incentives of these zones have remained in effect through the 2017 tax year and were extended for 2018 in March 2018 (see IRS Form 8444 at https://www.irs.gov/forms-pubs/about-form-8844).

[2] There are a few earlier documents on EZs available; however, the Virginia study appears to be the first to conduct an actual evaluation of the programme with an analysis of impacts.

[3] The comprehensive review of the What Works Centre for Local Growth (2016) identified 58 studies with robust impact evaluations of EZs.

[4] See Hooton (2016) for a comprehensive look at this issue.

[5] The citations provided are examples and do not represent all studies with this finding/conclusion.

[6] The US Federal EZ programme was approved in 1993 with funding starting in 1994 for 71 approved sites in six cities. Each of the sub-types is targeted using census tract units with selection criteria based on poverty levels and deprivation (Ferrara, 1982; Hebert et al., 2001; Hirasuna and Michael, 2005; Joint Committee on Taxation, 1981).

[7] The local, non-zone initiative in Miami also allows for such matching, while the other two local programmes require composite analysis areas.

[8] In its functional form the treatment variable T is a full set of interaction variables defined as $A_{izt} \times T_{izt}$ with an equal to 1 in those years when treatment occurs and T equal to 1 if the unit belongs to an analysis group that will at some point receive treatment.

[9] There is an estimate of an impact of approximately 1500% for the reduction of poverty population in Miami's EZ areas, but when comparing this estimate with manual calculations and negative binomial estimates conducted as robustness checks, the article believes this is due to noise in the data.

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Appendix A. Regression results

Table A1. *OLS impact estimates—Detroit*

Detroit

Full OLS Regression Impact Estimates

	Empowerment Zone				Renewal Zone				Renaissance Zone			
	DD Geo (1b)	DDD Geo (2b)	DD Non (3b)	DD City (4b)	DD Geo (1b)	DDD Geo (2b)	DD Non (3b)	DD City (4b)	DD Geo (1b)	DDD Geo (2b)	DD Non (3b)	DD City (4b)
Population	–0.025	–0.001	0.001	0.008	**–0.032*****	**–0.127*****	**0.031*****	–0.005	–0.003	0.016	–0.021	–0.023
	(*0.022*)	(*0.001*)	(*0.011*)	(*0.015*)	(***0.010***)	(***0.017***)	(***0.009***)	(*0.015*)	(*0.049*)	(*0.056*)	(*0.039*)	(*0.043*)
Poverty Rate (%)	**–0.047*****	–0.014	**–0.042*****	**–0.042*****	**–0.023*****	**–0.104*****	**–0.026*****	**0.060*****	–0.033	–0.014	**–0.082*****	**–0.081*****
	(***0.018***)	(*0.018*)	(***0.015***)	(***0.014***)	(***0.009***)	(***0.016***)	(***0.008***)	(***0.015***)	(*0.035*)	(*0.040*)	(***0.026***)	(***0.026***)
Poverty Population	**–0.239***	–0.002	**–0.121***	**–0.120***	**–0.231*****	**–0.848*****	**–0.122*****	–0.089	0.063	0.158	–0.128	–0.119
	(***0.136***)	(*0.081*)	(***0.073***)	(***0.073***)	(***0.041***)	(***0.122***)	(***0.034***)	(*0.066*)	(*0.115*)	(*0.141*)	(*0.107*)	(*0.100*)
Median HH Income	0.190	0.180	0.039	0.045	0.019	0.019	**0.061*****	0.068	0.168	0.0142	0.190	0.214
	(*0.134*)	(*0.173*)	(*0.150*)	(*0.174*)	(*0.017*)	(*0.019*)	(***0.019***)	(*0.047*)	(*1.113*)	(*0.836*)	(*0.317*)	(*0.358*)
Employment	n/a	n/a	**0.009*****	**0.005*****	n/a	n/a	**0.007*****	**0.444*****	n/a	n/a	**–0.004*****	**–0.014*****
			(***0.002***)	(***0.001***)			(***0.000***)	(***0.017***)			(***0.001***)	(***0.005***)
Establish-ments	n/a	n/a	0.000	0.481	n/a	n/a	**2.272*****	**0.597*****	n/a	n/a	**0.001*****	**0.002*****
			(*0.000*)	(*0.448*)			(***0.096***)	(***0.056***)			(***0.000***)	(***0.000***)
Resident Jobs	**0.099*****	–0.010	**0.005*****	**0.007*****	**–0.042*****	**0.254****	**–0.222***	0.094	**0.136*****	**–0.474*****	**0.051*****	**0.072*****
	(***0.018***)	(*0.007*)	(***0.008***)	(***0.010***)	(***0.016***)	(***0.100***)	(***0.122***)	(*0.095*)	(***0.113***)	(***0.138***)	(***0.019***)	(***0.027***)
Non-ResidentJobs	0.088	0.097	**0.302*****	**0.654*****	–1.790	–2.204	–2.077	–3.326	1.352	2.212	1.392	0.706
	(*0.105*)	(*0.333*)	(***0.101***)	(***0.219***)	(*1.592*)	(*2.590*)	(*1.512*)	(*4.104*)	(*5.482*)	(*6.865*)	(*5.644*)	(*2.863*)
Observations	4958											

Note: Each entry gives the regression estimate of programme designation cumulative impact on outcome variables for respective programme and start year. Column 1 reports DD estimates using a geographic buffer; column 2 reports DDD estimates using two geographic buffers; column 3 reports DD estimates using non-treatment tracts; column 4 reports DD estimates using citywide figures. Standard errors are shown in parentheses.
Asterisks reflect significance level obtained: ***1% level; **5% level; *10% level.[AU: Please provide suitable wording for the table footnote to give the meaning of the bold/italic values cited in Table 6 and Appendix Tables A1–A3.]

Table A2. *OLS impact estimates—District of Columbia*

District of Columbia								
Full OLS Regression Impact Estimates								
	Enterprise Community/Enterprise Zone				H Street			
	DD Geo (1a)	DDD Geo (2a)	DD Non (3a)	DD City (4a)	DD Geo (1a)	DDD Geo (2a)	DD Non (3a)	DD City (4a)
Population	0.011	0.017	–0.026	–0.025	–0.004	n/a	–0.008	–0.009
	(*0.024*)	(*0.041*)	(*0.030*)	(*0.029*)	(*0.057*)		(*0.075*)	(*0.086*)
Poverty Rate (%)	–0.009	–0.005	0.009	0.009	0.003	n/a	0.006	0.006
	(*0.015*)	(*0.027*)	(*0.018*)	(*0.018*)	(*0.027*)		(*0.029*)	(*0.030*)
Poverty Population	–0.197	–0.012	0.016	0.009	0.014	n/a	0.003	0.006
	(*0.929*)	(*0.139*)	(*0.126*)	(*0.067*)	(*0.147*)		(*0.184*)	(*0.341*)
Median HH Income	**–0.253*****	–0.225	**–0.268*****	**–0.444*****	–0.180	n/a	–0.157	–0.177
	(***0.081***)	(*0.144*)	(***0.053***)	(***0.039***)	(*0.171*)		(*0.140*)	(*0.158*)
Employment	n/a	n/a	**–0.021*****	**–0.073*****	n/a	n/a	**0.061*****	**0.093*****
			(***0.006***)	(***0.021***)			(***0.004***)	(***0.008***)
Establishments	n/a	n/a	**–0.002*****	**–0.206****	n/a	n/a	**–0.001*****	**–0.001***
			(***0.001)***	(***0.077***)			(***0.000***)	(***0.001***)
Resident Jobs	**–0.017****	0.186	**–0.208***	**–0.125***	–0.048	n/a	–0.045	–0.048
	(***0.008***)	(*0.168*)	(***0.114***)	(***0.068***)	(*0.044*)		(*0.065*)	(*0.069*)
Non-Resident Jobs	**–0.002*****	0.010	**–0.014****	**–0.009****	–0.009	n/a	–0.009	–0.009
	(***0.001)***	(*0.007*)	(***0.006***)	(***0.004***)	(*0.009*)		(*0.008*)	(*0.008*)
Observations	2493							

Note: Each entry gives the regression estimate of programme designation cumulative impact on outcome variables for respective programme and start year. Column 1 reports DD estimates using a geographic buffer; column 2 reports DDD estimates using two geographic buffers; column 3 reports DD estimates using non–treatment tracts; column 4 reports DD estimates using citywide figures. Standard errors are shown in parentheses.
Asterisks reflect significance level obtained: ***1% level; **5% level; *10% level.

Table A3. *OLS impact estimates—Miami*

Miami								
Full OLS Regression Impact Estimates								
	Enterprise Community/Empowerment Zone				Targeted Urban Area			
	DD Geo (1c)	DDD Geo (2c)	DD Non (3c)	DD City (4c)	DD Geo (1c)	DDD Geo (2c)	DD Non (3c)	DD City (4c)
Population	–0.025	0.008	–0.034	–0.036	**–0.051*****	–0.003	**–0.028***	**–0.045****
	(*0.037*)	(*0.060*)	(*0.038*)	(*0.045*)	(***0.017***)	(*0.032*)	(***0.016***)	(***0.019***)
Poverty Rate (%)	**–0.042***	–0.028	**–0.049*****	**–0.053*****	–0.001	0.013	0.005	0.005
	(***0.022***)	(*0.036*)	(***0.017***)	(***0.018***)	(*0.009*)	(*0.018*)	(*0.009*)	(*0.010*)
Poverty Population	–0.051	0.013	0.065	**–15.004***	**–0.105****	–0.039	0.000	0.000
	(*0.075*)	(*0.131*)	(*0.040*)	(***8.990***)	(***0.051***)	(*0.128*)	(*0.000*)	(*0.000*)
Median HH Income	–0.324	1.331	0.452	–1.417	**–0.197*****	–0.041	–0.003	–0.017
	(*0.997*)	(*3.986*)	(*0.738*)	(*0.861*)	(***0.562***)	(*0.272*)	(*0.003*)	(*0.015*)
Employment	n/a	n/a	**0.000*****	**0.000*****	n/a	n/a	0.000	**0.000*****
			(***0.000***)	(***0.000***)			(*0.000*)	(***0.000***)
Establishments	n/a	n/a	**0.000*****	**0.000*****	n/a	n/a	0.000	**0.000*****
			(***0.000***)	(***0.000***)			(*0.000*)	(***0.000***)
Resident Jobs	**0.067*****	**1.608*****	0.032	–0.070	**–0.016*****	**0.760****	0.058	0.024
	(***0.016***)	(***0.386***)	(*0.047*)	(*0.049*)	(***0.006***)	(***0.300***)	(*0.036*)	(*0.025*)
Non-Resident Jobs	0.013	**0.057***	0.003	–0.003	**0.010*****	**0.347****	**0.010***	**0.009***
	(*0.009*)	(***0.030***)	(*0.007*)	(*0.007*)	(***0.003***)	(***0.163***)	(***0.005***)	(***0.003***)
Observations	3196							

Note: Each entry gives the regression estimate of programme designation cumulative impact on outcome variables for respective programme and start year. Column 1 reports DD estimates using a geographic buffer; column 2 reports DDD estimates using two geographic buffers; column 3 reports DD estimates using non-treatment tracts; column 4 reports DD estimates using citywide figures. Standard errors are shown in parentheses.
Asterisks reflect significance level obtained: ***1% level; **5% level; *10% level.

Table A4. *Summary table of significant impact results and environmental factor variation*

		Results found	OLS sig. level	Total impact est.	Years of impl. or obs.	Avg. annual impact	Absolute avg. change in target	Absolute avg. change in city
Detroit Empowerment Zone								
Poverty Rate	DD Geo	Good	99%	−0.047	16	−0.003	Decrease	
	DD Non	Good	99%	−0.042	16	−0.003		Increase
	DD City	Good	99%	−0.042	16	−0.003		
Poverty Population	DD Geo	Good	90%	−0.239	16	−0.015	Increase	
	DD Non	Good	90%	−0.121	16	−0.008		Increase
	DD City	Good	90%	−0.120	16	−0.008		
Employment	DD Non	Good	99%	0.009	18	0.001	Decrease	Decrease
	DD City	Good	99%	0.005	18	0.000		
Resident Jobs	DD Geo	Good	99%	0.099	10	0.010	Decrease	
	DD Non	Good	99%	0.005	10	0.001		Decrease
	DD City	Good	99%	0.007	10	0.001		
Non-Resident Jobs	DD Non	Good	99%	0.302	10	0.030	Decrease	Decrease
	DD City	Good	99%	0.654	10	0.065		
Detroit Renewal Zone								
Population	DD Geo	Bad	99%	−0.032	16	−0.002	Decrease	Decrease
	DDD Geo	Bad	99%	−0.127	16	−0.008		
	DD Non	Bad	99%	−0.031	16	−0.002		
Poverty Rate	DD Geo	Good	99%	−0.023	16	−0.001	Increase	Increase
	DDD Geo	Good	99%	−0.104	16	−0.007		
	DD Non	Good	99%	−0.026	16	−0.002		
	DD City	Bad	99%	0.060	16	0.004		
Poverty Population	DD Geo	Good	99%	−0.231	16	−0.014	Increase	Increase
	DDD Geo	Good	99%	−0.848	16	−0.053		
	DD Non	Good	99%	−0.122	16	−0.008		
Median HH Income	DD Non	Good	99%	0.061	16	0.004	Decrease	Increase
Employment	DD Non	Good	99%	0.007	18	0.000	Decrease	Decrease
	DD City	Good	99%	0.444	18	0.025		
Establishments	DD Non	Good	99%	2.272	18	0.126	Decrease	Decrease
	DD City	Good	99%	0.597	18	0.033		
Resident Jobs	DD Geo	Bad	99%	−0.042	10	−0.004	Decrease	Decrease
	DDD Geo	Good	95%	0.254	10	0.025		
	DD Non	Bad	90%	−0.222	10	−0.022		
Detroit Renaissance Zone								
Poverty Population	DD Non	Good	99%	−0.082	16	−0.005	Increase	Increase
	DD City	Good	99%	−0.081	16	−0.005		

Table A4. Continued

		Results found	OLS sig. level	Total impact est.	Years of impl. or obs.	Avg. annual impact	Absolute avg. change in target	Absolute avg. change in city
Employment	DD Non	Bad	99%	–0.004	18	0.000	Decrease	Decrease
	DD City	Bad	99%	–0.014	18	–0.001		
Establishments	DD Non	Good	99%	0.001	18	0.000	Decrease	Decrease
	DD City	Good	99%	0.002	18	0.000		
Resident Jobs	DD Geo	Good	99%	0.136	10	0.014	Decrease	Decrease
	DDD Geo	Bad	99%	–0.474	10	–0.047		
	DD Non	Good	99%	0.051	10	0.005		
	DD City	Good	99%	0.072	10	0.007		
DC Enterprise Community/Enterprise Zone								
Median HH Income	DD Geo	Bad	99%	–0.253	16	–0.016	Increase	Increase
	DD Non	Bad	99%	–0.268	16	–0.017		
	DD City	Bad	99%	–0.444	16	–0.028		
Employment	DD Non	Bad	99%	–0.021	18	–0.001	Increase	Decrease
	DD City	Bad	99%	–0.073	18	–0.004		
Establishments	DD Non	Bad	99%	–0.002	18	0.000	Increase	Increase
	DD City	Bad	99%	–0.206	18	–0.011		
Resident Jobs	DD Geo	Bad	95%	–0.017	10	–0.002	Increase	Increase
	DD Non	Bad	90%	–0.208	10	–0.021		
	DD City	Bad	90%	–0.125	10	–0.013		
Non-Resident Jobs	DD Geo	Bad	99%	–0.020	2	–0.010	Increase	Increase
	DD Non	Bad	95%	–0.014	2	–0.007		
	DD City	Bad	95%	–0.009	2	–0.005		
DC H Street Corridor								
Employment	DD Non	Good	99%	0.061	2	0.031	Increase	Decrease
	DD City	Good	99%	0.093	2	0.047		
Establishments	DD Non	Good	99%	0.001	2	0.001	No change	Increase
	DD City	Good	95%	0.001	2	0.001		
Miami Enterprise Community/Enterprise Zone								
Poverty Rate	DD Geo	Good	90%	–0.042	16	–0.003	Decrease	Decrease
	DD Non	Good	99%	–0.049	16	–0.003		
	DD City	Good	99%	–0.053	16	–0.003		
Poverty Pop.	DD City	Good	90%	–15.004	16	–0.938	Decrease	Decrease
Employment	DD Non	Neutral	99%	0.000	18	0.000	Decrease	Decrease
	DD City	Neutral	99%	0.000	18	0.000		

Table A4. Continued

		Results found	OLS sig. level	Total impact est.	Years of impl. or obs.	Avg. annual impact	Absolute avg. change in target	Absolute avg. change in city
Establishments	DD Non	Neutral	99%	0.000	18	0.000	Increase	Increase
	DD City	Neutral	99%	0.000	18	0.000		
Resident Jobs	DD Geo	Good	99%	0.067	10	0.007	Increase	Increase
	DDD Geo	Good	99%	1.608	10	0.161		
Non-Resident Jobs	DDD Geo	Good	90%	0.057	10	0.006	Decrease	Decrease
Miami Targeted Urban Area								
Population	DD Geo	Bad	99%	–0.051	16	–0.003	Decrease	Decrease
	DDD Geo	Bad	90%	–0.028	16	–0.002		
	DD Non	Bad	95%	–0.045	16	–0.003		
Poverty Pop.	DD City	Good	95%	–0.105	16	–0.007	Decrease	Decrease
Median HH Inc.	DD City	Bad	99%	–0.197	16	–0.012	Increase	Increase
Employment	DD City	Neutral	99%	0.000	18	0.000	Decrease	Decrease
Establishments	DD City	Neutral	99%	0.000	18	0.000	Increase	Increase
Resident Jobs	DD Geo	Bad	99%	–0.016	10	–0.002	Decrease	Increase
	DDD Geo	Good	95%	0.760	10	0.076		
Non-Resident Jobs	DD Geo	Good	99%	0.010	10	0.001	Decrease	Decrease
	DDD Geo	Good	95%	0.347	10	0.035		
	DD Non	Good	90%	0.010	10	0.001		
	DD City	Good	90%	0.009	10	0.001		